SCIENCE.

FRIDAY, JUNE 17, 1887.

COMMENT AND CRITICISM.

THE IMMIGRATION INTO THIS COUNTRY during a year is so enormous, that we are apt to overlook the fact that similar movements of population may be taking place elsewhere. To be sure, immigration elsewhere is very small as compared with that here, but it has attracted sufficient notice in England of late to call parliament's attention to it. Investigation proves, however, that any alarm which may have been caused is unnecessary. Comparison of the census of 1881 with that of 1871 shows that the immigration of foreigners into the United Kingdom during that decade cannot have been very large. According to tables which have been prepared, the increase of foreigners resident in the United Kingdom between 1871 and 1881 was from 113,979 to 135,640, or 21,661 in all, equal to just over 2,000 per annum. Having regard to the figures of emigration and immigration dealt with in the board of trade tables, this is of course a small movement. The whole foreign population resident in the United Kingdom in 1881 was in fact less than the net emigration of British and Irish persons from the United Kingdom in a single year. The German empire contributed 35,141 in 1871, and 40,371 in 1881; France, 19,618 in 1871 against 16,194 in 1881; Russia, 9,974 against 15,271; and the United States, 9,467 against 20,014. Thus Germans constituted in 1881 about one-third of the foreign population resident in the United Kingdom; but the increase in the period was no greater than the increase among Russians, and less than the increase among natives of the United States, whose numbers doubled in the ten years. It seems probable that the increase of foreigners since 1881 has been somewhat more rapid than during the decade preceding, but it has not yet become so great as to be at all alarming.

NO PAPER THAT WAS PRESENTED at the recent successful session of the Historical and Economic associations at Boston was more important than that by Col. Carroll D. Wright on 'The study of statistics in colleges.' What he said about the necessity for the scientific study of statistics and their application should be specially emphasized. Colonel Wright, himself a most successful statistician, avowed that during the fourteen years that he had devoted to practical statistics there had not been a single day when he had not felt the need of statistical training, not only for himself, but for those associated with him. He continued, "The problems which the statistician must solve, if they are solved at all, are pressing upon the world. Many chapters of political economy must be rewritten; for the study of political economy is now brought under the historical and comparative method, and statistical science constitutes the greatest auxiliary of such a method. There is so much that is false that creeps into the popular mind, which can only be rectified through the most trustworthy statistical knowledge, that the removal of apprehension alone by it creates a necessity sufficient to command the attention of college authorities. The great questions of the day, the labor-question, temperance, tariff reform, all great topics, demand the auxiliary aid of scientific statistics, and a thorough training is essential for their proper use." Two instances were cited by Colonel Wright to show the way in which crude theories are sometimes upset by carefully gathered statistics: "It has been asserted that there is an alarming amount of illiteracy in Massachusetts. Statistical inquiry shows that by far the greater number of these illiterates are of foreign birth; so that the fault is not with the public-school system, but the evil is due to a temporary cause, namely, immigration. Again: it has been freely asserted that in the United States, women of native birth do not have as many children as women of foreign birth, and that thereby the real American population is steadily losing ground. The census of Massachusetts will show, that although American women do have a less number of children, on the average, yet a larger number survive, so that the alarm is needless. Common observation would never have shown these things, or would not have shown them accurately."

We fancy that the average reader of census-tables has little conception of the many difficul-

ties, purely statistical, which must be surmounted before the tables are completed. Colonel Wright drew from his own experience excellent illustrations of these. "The question may be asked," he said, "what elements of capital are involved in the census question of 'capital invested'? Is it simply the cash capital invested by the concern under consideration, or is it all the money which is used to produce a given quantity of goods? If the members of a firm contribute the sum of $10,000, and they have a line of discounts of $100,000, the avails of which are used in producing $200,000 worth of completed goods, what is the capital invested? What is the capital invested which should be returned in the census? If a man has $5,000 invested in his business as a manufacturer, and he buys his goods on ninety days, or four months, and sells for cash, or thirty days, what is his capital invested? This question is one among many of the practical problems that arise in a statistical bureau, but which has not yet been treated scientifically. What has been the result of the reported statistics relating to capital invested? Simply that calculations, deductions, and arguments based on such statistics have been and are vicious, and will be until all the elements involved in the term are scientifically classified. Another illustration in point arises in connection with the presentation of divorce statistics, especially when it is desired to compare such statistics with marriages, or to make comparisons to show the progress, or the movement of divorces. Shall the number of divorces be compared with the number of marriages celebrated in the year in which the divorces are granted, or with the population, or with the number of married couples living at the time? I need not multiply illustrations. The lies of statistics are unscientific lies." In speaking of the U. S. census, Colonel Wright said, that although we take a census in the United States every ten years, yet, as a rule, the men that are brought into the work know nothing of statistics. They should be trained in the very elementary work of census-taking and of statistical science. It would be much more economical for the government to keep its experienced statisticians busily employed in the interim of census-taking, even if they do no more than study forms, methods, and analyses connected with the presentation of the facts of the preceding census. Money would be saved, results would be more thoroughly appreciated, and problems would be solved. The next congress must make the preliminary arrangements for the eleventh census, and it would be a national gain were Colonel Wright himself put in charge of the work.

PHYSICAL CULTURE FOR CRIMINALS.

IN *Science* for May 13 appeared a favorable notice of an experimental class in physical culture, conducted during the summer of 1886 at the New York state reformatory, and described at length in the last annual report of the board of managers. The class consisted of twelve men, dull and stupid, but not idiots or imbeciles, who seemed incapable of any prolonged mental effort, and who had failed to make any appreciable progress in school-work. The object in view in the formation of the class was to determine if physical culture, with all that the term implies, would not result in at least a partial awakening of dormant mental power in twelve men mentally and morally obtuse.

With physical culture and improvement, there came a mental awakening; and at the end of five months, when the class was discontinued, the men were able to perform operations in simple arithmetic, as division and cancellation, — a thing they had never done before, as the average criminal is remarkably dull in all that pertains to mathematics.

It is now more than six months since the class was given up, and the men assigned to various shops and employments and the primary classes of the reformatory, — a period sufficiently long to determine, in part at least, the value of physical culture as an educational factor.

One man, a southern negro, died during the winter from pulmonary disease, leaving eleven men under observation at the present time. At the time the class was formed nine of these eleven men were in the third grade, and two in the second or intermediate. Five months later, or when the class was discontinued, these nine men had attained the second grade, and the two there originally had maintained their standing. At the present time of writing, six have reached the first grade, leaving five in the second; and of these latter, two have every prospect of reaching the first by the beginning of May.

The average marking of these eleven men for the six months preceding their course of training, and while engaged in shop-work, was as follows: demeanor,— $2\frac{1}{2}$; labor, $2\frac{10}{33}$; school, $1\frac{3}{22}$, or 46 per cent; 3 representing the highest attainable mark in each, or an aggregate of 9 for the time named. During the continuance of the class, and in response to the efforts made to arouse these men

from their state of mental lethargy, their marking in school rose to 74 per cent, and their demeanor proportionately improved. From November, 1886, to April, 1887, inclusive, the men being employed as laborers and at various industries, as brush-drawing, their average marking was as follows: demeanor, $2\frac{1}{5}$; labor, $2\frac{9}{11}$; school, $2\frac{9}{30}$, or 71 per cent, — a great improvement as compared with their record from December, 1885, to May, 1886, inclusive, as given above. The record of these eleven men for corresponding periods before and after their course of physical training presents a marked contrast.

If the improvement noted in these dullards during the time they were receiving their athletic training was the result of better spirits, arising from the novelty of their position, and pride that they were singled out from their fellows for certain work, and removed in a measure from prison monotony, it would be reasonable to expect that with the removal of the stimulus, and the return of all to the routine prison-life, with the consequent loss of the individuality they might have enjoyed, there would come sooner or later a falling-back and lapsing into their previous state of mental inertia. But, returned to the *régime* and discipline observed with other prisoners, they maintained their good record; and, six months after the termination of the experiment, the mental power revealed by their physical-culture course has continued to develop, and the former shuffling gait and stooping shoulders which characterized them as a class have been replaced by an alertness and promptitude of action.

I do not think the improved mental condition of these men can be attributed to other than the strengthening of the brain-centres by the cultivation and development of muscle and muscles under the control of these same nervous centres, the one participating and taking part in the improvement of the other. From the words of commendation I have received, and noting the progress of the men under conditions that once seemed to promise so little to them by reason of their stupidity and obtuseness, I regard my class in physical culture as more than an experiment, — a success, — as showing that something more than mere brawn can be accomplished by muscular exercise when properly selected, guided, and governed. H. D. WEY, M.D.

DISTILLERY-MILK REPORT.[1] — *II.*

IN response to the question, What is your opinion as to the wholesomeness of distillery swill as food for cows? the following were received: —

[1] Continued from p. 553.

[D. W. HAND, M.D.]

I do not believe it to be a wholesome food.

[L. MCLEAN, M.R.C.V.S.]

Detrimental to the general health of any ruminating animal. As such food does not require to be masticated, or remasticated, hence a perverted condition of the ruminating apparatus.

[EDWARD PLAYTER, M.D., editor of the Canadian *Health journal.*]

I have observed a number of items in medical journals (of which I, as editor for twelve years of the Canadian *Health journal*, have received many), referring to the injurious effects of the swill upon the milk of milch-cows fed with it, but I cannot call to mind any facts. Knowing well the effects of dirt upon the organs and secretions of both man and animals, I am convinced that distillery swill, which must constitute a very imperfect food, would furnish but a very inferior milk, and that cows fed chiefly or largely upon such swill give a milk of inferior quality, and not fit for habitual use, especially as food for infants. Animal chemistry and physiology would seem to render this impossible.

[CHARLES SCHAEFFER, M.D., Philadelphia, Penn.]

Upon general principles, I judge that food which breaks down the cow's constitution, very much as chronic alcoholism (which does not result in fatty degeneration) destroys the human constitution, producing diarrhoea and muscular atrophy, is not likely to give a very healthy secretion of milk, but, on the contrary, a poisonous one.

[OSCAR C. DEWOLF, M.D., Chicago, Ill.]

I have been commissioner of health of the city of Chicago for eleven years past, and during that period, until 1885, several hundred milch-cows were constantly fed in distillery sheds in this city. I believe that distillery slop before it has passed into the acetic acid fermentation, and fed in proper quantities to cows running at large, is perfectly wholesome food. I object to so-called 'distillery milk,' because of the close and long confinement of cows, and the dirty methods of gathering and storing the milk. It is probable, also, that cows thus confined do not often receive the quantity of hay they require for vigorous health. These conditions must affect the milk, whether chemists can detect the change or not. Not a cow giving milk for public supply is now fed and confined in a distillery shed in this city, and for reasons above given I shall oppose any attempt to do so.

[WILLIAM OLDWRIGHT, M.D.]

I consider distillery swill an unwholesome food for cows.

WILLIS G. TUCKER, M.D., professor of inorganic and analytical chemistry, Albany medical college.]

I am opposed to the use of such waste as a chief or exclusive diet for milch-cattle, though I do not believe that the feed is the sole cause of disease among cattle in swill-stables, or of the poor milk furnished by them.

[E. H. BARTLEY, M.D.]

Unwholesome both to cows, and to the children fed upon the milk.

[WILLIAM K. NEWTON, M.D.]

I am of the opinion that it is an unwholesome food, and that the milk produced by cows using it is not healthful.

There has been a great deal on this subject published, and all the German authorities agree that stall-fed cows give as good milk as those allowed to graze, due attention being paid to the sanitary condition. In many German cities the milk-supply is obtained from cattle thus cared for. In the last two reports of the Wisconsin experiment-station, very interesting accounts are given about soiling cows; and the results, as to yield and quality of the milk, are nearly the same as from cows allowed to feed in the pasture. In the soiling method the food is all given to the cows in the stalls, and they are only allowed in the barnyard for exercise, and on clear days. The cows are turned into milk-making machines.

In the case of distillery swill, the cattle are not only fed on an unnatural food, but are at the same time subjected to very unsanitary conditions; and both combined cause disease, and hence the product must, of necessity, be unhealthful. I am pretty certain that those scientific men who are willing to indorse this business are either not acquainted with the subject, or confound proper soiling with the methods in vogue at Blissville. The two systems are separate and distinct.

[J. BLAKE WHITE, M.D.]

Positively unwholesome.

[GEORGE H. ROHÉ, M.D.]

I have no hesitation in saying that distillery swill is not only unhurtful, but desirable as food for dairy cattle. I have seen no trustworthy evidence that the bad results of stall-feeding in dairies are due to this food. I would desire to express my opinion as emphatically as possible upon this point.

[PROF. WILLIAM H. BREWER.]

I have an *opinion*, founded on reading rather than observation, that milk from cows fed principally or largely on distillery swill is *decidedly unwholesome*, but that distillery swill may be used in small quantities, along with other food, without seriously or demonstrably deteriorating the wholesomeness of milk; that the evil effect is largely a matter of relative quantity of swill to other food. Moreover, the surroundings of the cows in swill-milk stables as usually kept, and also the health of the cows as usually found in those stables, is, or are, factors causing much of the alleged unwholesomeness. Milk is an easy carrier of smells and disease.

[HENRY HARTSHORNE, M.D.]

My supposition is, that it is very likely to contain a remnant of alcohol, and that this must interfere with its suitableness for cattle-food. If this be so, it is also *possible* that a small portion of alcohol may pass through the cow's blood into the milk, to the injury of infants fed upon it. But such possibilities are only sufficient to justify careful *investigation*. At the best, however, such material is obviously very far removed from the condition of natural food for cows.

[E. M. NELSON, M.D.]

I think it is not a wholesome food, and that the milk from swill-fed cows is excessively acid, decomposes early, and predisposes to disturbances of digestion.

[W. SIMON, PH.D.]

My opinion, based on my examination in 1882 and numerous observations made in various localities since that time, is that 'swill,' when used in moderate quantities alongside of plenty of hay, grass, or other similar food, is a highly valuable article for feeding cattle. On the other side, swill becomes dangerous when fed in too large quantities, most likely on account of its high percentage of nitrogenous matter.

[CHARLES AMBROOK, M.D., Boulder, Col.]

If made an exclusive diet, unwholesome; if not exceeding one-quarter of whole diet, and good pasturage always at hand, nothing very detrimental in distillery food that I have seen.

[A. J. HOWE, M.D., Cincinnati, O.]

Distillery slop blackens the teeth of kine, — cows or oxen, — makes their breath offensive, gives them diarrhoea, and weakens the muscular system to a degree that, though fat, the creatures can hardly walk. The above I know from observation.

[NORMAN S. BRIDGE, M.D.]

That it is an unnatural food; almost sure, sooner or later, to cause some disease in the cows, unless it is freely mixed with a large quantity of other and more natural food. Doubtless the complaints referred to under No. 2 were mainly in

cases where the milk used was from cows the health of which had undergone some deterioration from the diet referred to.

[J. L. HAMILTON, M.D., Peoria, Ill.]

Since our dairies have been removed to the country, and the cows fed on other food, and some slop still used, the effect of the still-slop is not noticed. Of course, there are other things as well as the slop. When cows are kept up in barns, and fed only on still-slop, the air they breathe is very impure, and they will drink but little water and have no exercise. This contributes to the unhealthiness of dairy milk.

[C. A. ROHILLARD, M.D.]

Knowing that this matter is extensively used in some parts for fattening purposes, and that healthy beef is brought to the market as a result of this mode of feeding, I would incline to the belief that the milk from cows so fed is all right. I am not prepared, however, to state positively that it should be so under all circumstances.

[JAMES E. REEVES, M.D., Wheeling, W.Va., formerly secretary state board of health.]

My observation, from the stand-point of the general practitioner of medicine, fully warrants the belief that the milk of town-fed cows — feeding on slops, garbage, and brewery refuse — is dangerous to the public health.

[HENRY D. HOLTON, M.D., Brattleborough, Vt.]

Here in Vermont we do not have any thing of the kind; yet we are well aware that the food of the cow has much to do with the quality of the milk and butter. In summer, dairymen know from experience and observation that there is a great difference in the pastures. When cows are in some pastures, the milk, and especially the butter, is much better than when in others. Many people can tell butter made when the cows are fed on cottonseed-meal instead of corn-meal. It is also true that the milk of cows who are worried or frightened will sour much quicker than when not so worried. Infants fed with the milk of cows worried or heated by running (as is sometimes done by boys in bringing them from the pasture) will suffer from colic, and often from diarrhoea. There is no doubt in my mind that swill from distilleries would produce a very poor quality of milk.

[D. L. PHARES, M.D., member of state board of health, Agricultural college P.O., Miss.]

That it is unwholesome. In small quantity, combined with plenty of good, sound normal cow-food, it may do no serious injury; but in any considerable quantity it is, in my opinion, unwholesome. The nature and condition of the substance seem to me to justify this opinion. True, it may for a time seem to improve the condition of the cow, but even then the physiologist and pathologist can detect evidences of damage.

[G. A. LIEBIG, Baltimore, Md.]

I would unhesitatingly prefer other than milk from cows so fed, not only for the reason of character of food, but also for the manner of treatment of the animals, — housing, etc.

[R. HARVEY REED, secretary Ohio state board of health.]

I think distillery swill is very objectionable food.

[L. M. KENYON, M.D. Buffalo, N.Y.]

I think, from what I have read from time to time, and know from what little I have seen, that it is most decidedly detrimental, although I can now give no detail, or specially individual cases.

[J. F. KENNEDY, M.D., secretary Iowa state board of health, Des Moines, Io.]

Upon general principles, I should consider such food as injurious to the cows, and hence productive of milk injurious to those using it, especially to children largely dependent upon it.

[F. N. BOKER, sanitary engineer, Montreal, Can.]

Decidedly unwholesome. It soon acquires a rotten flavor, and is deceptive as to nourishment. During our long Canadian winter in Montreal, a good deal of swill is given to milch-cows to increase the flow of milk; and, as the mortality among young children is very great in this city, I attribute it to the poor quality of the milk, etc.

[*To be continued.*]

EXPLORATION AND TRAVEL.

Lieutenant Wissmann's expedition.

IN *Science* of April 22 we referred to Lieutenant Wissmann's trip from Luluaburg to the Lubilash. A letter from Wissmann which was published in the *Verhandlungen der Gesellschaft für Erdkunde*, April, 1887, contains the following interesting details. He ascended the Lulua as far as Katende (the situation of which may be seen on our map of Central Africa). Here he visited the grand Lulumba Falls, which are the termination of the navigable part of the Lulua. He had some difficulty in crossing the river, on account of the hostility of the natives. He proceeded eastward, and, after crossing the river Moio on a bridge, reached Tenda-Mota. Here is the boundary between the Bashilange and Bagna-Kalosh, who belong to the Baluba. The Kalosh and their eastern neighbors live in small villages of from four to ten houses, which are surrounded by fields in which they

grow sweet-potatoes, hirse, and manioc of a poor quality. There is scarcely any uncultivated land, one field adjoining the other, and one village being close to the other. Wherever a patch of uncultivated land exists, it is prairie, with scattered shrubs three feet in height. The land is not very fertile, and does not yield large crops. On the steep knolls which form the watersheds there are a few large trees. The banks of brooks and rivers are barren, and in some places the hills and plains are covered with granite bowlders. Very few bananas are grown in the villages. The men are very tall, and have heavy bones. They wear head-dresses made of feathers, and have their hair arranged in a thick knot on the back part of the head, and in numerous small knots in front. Their spears are generally made of hard wood: they always carry a club, and use the broad knife of the Lunda. Wissmann considers them one of the finest-looking peoples of Central Africa.

It was impossible to buy any thing, as the population was too dense. Small-pox is endemical. On the Buchimayi, a western tributary of the Lubilash, the natives attacked the caravan, and Wissmann was compelled to return to Luluaburg. In October, 1886, he started on his journey to the unknown district between the Sankuru and the upper Kongo. He writes that the natives informed him of the existence of lakes similar to Lake Mantumba and Lake Leopold in this region. It is worth remarking, that, according to Dr. Wolf's observations, the Sankuru has no tributaries on its right bank. There are only a few small brooks, which have black water. This shows that they come from a swampy region. The Busera, Juapa, and Lubilash, on the other hand, which come from the same region, have water of a light yellowish color. Wissmann intends to explore this watershed, and to reach the Kongo near Nyangwe.

In regard to the Bashilange and Bateke tribes, Wissmann says that probably Baluba, who emigrated from the upper Lubilash, intermarried with a people similar to the dwarfish Watwa. Of these, the Bashilange and Batcke are the descendants. In their districts no tribe of dwarfish stature exists, while they may be found among the Bakuba, Basonge, Wanyēma, and Baluba. The Baluba occupy the whole territory as far east as the Tanganyika, Lukuga, and Lake Meru. The King of Lunda, the Muata Yamvo, is of Baluba descent. The remarks on the anthropological features of the Bashilange agree with the views of R. Virchow, expressed some months ago (*Verh. der anthrop. Ges.*, Berlin, 1886), when discussing the valuable anthropological measurements and the skulls collected by Dr. Wolf on his memorable journeys in Central Africa. Virchow says that the anthropological features of these tribes are those of a mixed race, the negro type prevailing. He does not express an opinion as to the second element. His conclusions are supported and completed by Wissmann's ethnological observations on the non-existence of a dwarfish population in the territories inhabited by the Bashilange.

Asia.

General Ignatief, governor of eastern Siberia, has proposed the exploration of part of the frontiers between Russia and China. A large expedition is being equipped, which is to visit the Saïansky Mountains and the Kossogol west of Irkutsk. Colonel Bobyz is the leader of the expedition, which will last from five to six months (*Gaz. géogr.*, May 19).

The Imperial geographical society of St. Petersburg proposes to study the periodical changes and the gradual desiccation of the lakes of western Siberia. The plan of the work is designed by Potanin, Yadriutzef, and other Russian explorers of northern Asia, the president of the committee being Mr. Stebnitzky.

Mr. B. C. Henry has made a second visit to the Island of Haiman. He visited the aborigines of the mountain region, reaching the geographical centre of the Lee territory, and demonstrating the fact that this region, supposed to be impassable, can be traversed from east to west and from north to south with comparative ease (*Proc. Roy. geogr. soc.*, June).

Africa.

A Reuter's telegram from S. Paul de Loanda, dated May 26 (*Scottish geogr. mag.*, June), announces the arrival of Mr. Stanley's expedition at Leopoldville on April 20, all well, and the departure of the main body nine days later.

Le mouvement géographique publishes a brief description of the exploration of the river Inkissi, which empties into the Kongo near Stanley Pool, coming from the south. The explorer, Lieutenant Hakansson, started on his expedition on the 6th of November. For three days he passed through a barren desert, but then the country became more fertile and settled. This observation is of some importance on account of the disputed extent of the barren district on the west coast of Africa. From all observations, it appears that the region of the lower Kongo, though generally very dry and barren, contains numerous patches of fertile land. The population of the Inkissi consists mainly of Bakongo.

Mr. J. T. Last, who has followed up the work of Mr. O'Neill by exploring the Namuli Hills and the Lukugu valley, has arrived at Zanzibar. He

has carried out the programme of his journey, though he found the summit of the Namuli Hills inaccessible, and in addition traversed the whole region a second time, striking into the interior from Kwilimane, and emerging at Ibo on the Mozambique coast (*Proc. Roy. geogr. soc.*, June).

America.

Under the auspices of the Italian geographical society, Count Ermanno Stradelli from Piacenza, who has travelled for many years on the Amazon and its tributaries, is going to explore the head waters of the Orinoco, which were visited in the beginning of this year by Chaffanjon (*Boll. Soc. geogr. Ital.*, May).

Prof. Dr. R. A. Philippi writes to *Petermann's Mittheilungen* that the Chilian government has sent out two expeditions to survey the boundary between Chili and the Argentine Republic from Rio Palena to the pass of Villarica. It appears that the Cordillera is situated in Chilian territory, while the watershed between the Atlantic and Pacific oceans, which forms the boundary, lies east of the mountains, about 1,600 feet high. One of the expeditions will cross the Ranco pass east of Valdivia, and return by the pass of Villarica. The time allowed to the expedition is from two to two and a half months.

HEALTH MATTERS.

YELLOW-FEVER AT KEY WEST. — The existence of yellow-fever at Key West is officially recognized and declared epidemic by its board of health. In a proclamation issued by that body, it is stated that an effort is being made to conceal cases, and to resist the health officers. The board announces that a bulletin will each day at noon give the status of the epidemic, naming new cases, deaths, and recoveries. Reports are required from every householder of any sickness which may occur in his family. Unacclimated persons are required to remove from the infected district, and are advised to leave the island. Proprietors of saloons are especially called upon to refuse drinks to those inclined to abuse the use of the same, since such persons taken with fever are nearly hopeless cases, and their deaths add to the mortality list, and tend to increase mortality among others.

PLEURO-PNEUMONIA IN WESTCHESTER. — There has been an extensive outbreak of contagious pleuro-pneumonia among the cattle near Golden's Bridge, Westchester county, N.Y. In one of the affected herds there are two hundred and sixty head of cattle. In addition to this, several smaller herds are affected. The cattle have been appraised under the direction of the U. S. bureau of animal industry, of which Dr. D. E. Salmon is chief, and are being slaughtered. It is the hope of Dr. Salmon to eradicate the disease from the county.

NOTES AND NEWS.

AN *Archiv für Geschichte der Philosophie* will shortly appear in Berlin. The editor-in-chief is to be Prof. Ludwig Stein of Zurich.

— The *Athenaeum* announces that the well-known Swedish botanist, Prof. Johan Edvard Areschoug, died at Stockholm on the 7th of May. He was born in 1811, and worked under Agardh and Fries at Lund. He was made reader in botany at that university in 1839, and in 1858 was appointed to succeed Elias Fries as professor of botany at the University of Upsala. Among his numerous publications, those best known are his 'Symbolae algarum florae Scandinaviae,' his 'Iconographia phyctologia,' and his 'Phyceae marinae.' Areschoug retired from his chair in 1876. On the same day the Swedish statistical writer, Dr. Fredrik Theodor Berg, died in Stockholm, in his eighty-first year.

— Messrs. John Wiley & Sons, New York, have issued an admirable catalogue of their publications, which cover every department of the mathematical sciences and of engineering.

— The second number in the series of monographs on political economy and public law, edited by Prof. Edmund J. James, and published by the University of Pennsylvania, will shortly appear. It treats of the anti-rent riots in New York, 1839–46, an important but hitherto almost entirely neglected chapter in American economic history. The author, Mr. E. P. Cheyney, instructor of history in the University of Pennsylvania, finds the source of the difficulties, which in many respects resemble the present Irish land-troubles, in the peculiar land-tenures of early New York. A vivid description is given of the rise and progress of the riots, and a full account of the numerous and important changes in the constitution and laws of the state, which followed as a result of this movement.

— On Friday, May 13, the Hon. Ion Grant Neville Keith-Falconer died at Aden, and with him one of England's most promising scholars passed away. Mr. Keith-Falconer was born in 1856, and graduated at Trinity college, Cambridge, in 1878, attaining high honors in Semitic languages. After a period of study in Germany and the east, he became Hebrew lecturer at Clare college; and on the resignation of Professor Robertson Smith in June, 1886, he was appointed

lord-almoner's reader in Arabic for the University of Cambridge. His published writings are principally on philological topics; and the article on 'Shorthand,' in the 'Encyclopaedia Britannica,' is from his pen.

— The twenty-second volume of the 'Encyclopaedia Britannica,' completing the letter 'S,' is now ready. The principal literary and scientific articles are, 'The sonnet,' by Mr. Theodore Watts; 'Sophocles,' by Professor Campbell; 'Spanish literature,' by M. Morel Fatio, the first Spanish scholar in Europe; 'Swedish literature,' by Mr. Gosse; 'Syriac literature,' by Professor Wright; 'Dean Stanley,' by the present dean of Westminster; 'Socrates,' by H. Jackson; 'Stoics,' by D. Hicks; 'Slavs,' by Mr. Morfill; 'Slavery,' by Dr. Ingram; 'Skeleton,' by Prof. St. George Mivart; 'Sponges,' by Dr. Sollas; 'Steam-engine,' by Professor Ewing; 'Sun,' by Mr. Lockyer; 'Surface,' by Professor Cayley; 'Surgery,' by Professor Chiene and three other contributors; 'Spiritualism,' by Mrs. Henry Sidgwick; and 'Sword,' by Prof. F. Pollock.

— The issue of the *Home journal* dated June 15 contains a most complete summer-resort guide. Where to start from, how to go, what it costs to stay, the natural attractions of the different regions, and the accommodations offered by the various hotels at the summer-resorts, are all very clearly and faithfully set down.

— Hon. David A. Wells will contribute to the July *Popular science monthly* the first of an important series of papers on 'The economic disturbances since 1873.' Mr. Wells proposes to review the history of these disturbances, and to point out agencies to which such wide-reaching commercial depression may be properly attributed.

— The two latest monographs issued by the American historical association are 'History of the doctrine of comets,' by ex-President Andrew D. White of Cornell; and 'William Usselinx, founder of the Dutch and Swedish West India companies,' by Dr. J. F. Jameson of Johns Hopkins university.

— The progress made in educating the negroes of the south will be set forth in *The American magazine* for July. The Rev. S. W. Culver, president of Bishop college, Texas, describes the methods of instruction, and the measure of success attained.

— Prof. M. Max Müller's three lectures — 'The simplicity of language,' 'The identity of language and thought,' and 'The simplicity of thought' — given at the Royal institution, London, last March, have been secured for the columns of *The open court*, Chicago. The first of these remarkable lectures was contributed to the May number of the *Fortnightly review:* the other two have not been published, and will be printed for the first time in *The open court*, and from the author's manuscript. The publication of these lectures commenced in *The open court* of June 9.

— The *Harvard university bulletin* announces that the corporation have authorized the publication, through Charles Scribner's Sons, of a memorial edition of the late Prof. E. A. Sophocles' 'Greek lexicon of the Roman and Byzantine periods,' under the oversight of Prof. Joseph Henry Thayer.

— Charles L. Webster & Co., the publishers, sent Mrs. Grant a check for $33,384.53 last week as additional profits on General Grant's 'Memoirs.' She has received thus far nearly $400,000, which is probably the largest amount of money ever earned by the writing of a single book.

— Cupples & Hurd have in preparation a life of Commodore Matthew C. Perry, who was so instrumental in opening the ports of Japan to the world. It will give a complete history of this 'typical naval officer' from the time when, as a midshipman, he served in the war of 1812, to the treaty with Japan.

— Messrs. Macmillan & Co. have published 'Dynamics for beginners,' by Rev. J. G. Lock. This work has been written in the hope of supplying a want, which many teachers have felt, of a book which explains the elementary principles of dynamics, and at the same time illustrates them by numerous easy numerical examples suitable for use in schools with boys of ordinary mathematical attainments. It must be regretted, however, that the author has seen fit to suggest names for the units of velocity and acceleration, as the science of physics threatens to be overburdened with an unnecessary nomenclature.

LETTERS TO THE EDITOR.

**** The attention of scientific men is called to the advantages of the correspondence columns of* SCIENCE *for placing promptly on record brief preliminary notices of their investigations. Twenty copies of the number containing his communication will be furnished free to any correspondent on request.*

The editor will be glad to publish any queries consonant with the character of the journal.

Correspondents are requested to be as brief as possible. The writer's name is in all cases required as proof of good faith.

The Charleston earthquake.

The admirable paper on the 'Charleston earthquake' in *Science* of May 20, by Messrs. Dutton and Hayden, is an illustration of what may be accom-

plished by the patient and laborious investigation of a mass of evidence, much of which is unsatisfactory, and not a little of it untrustworthy. That the paper contains so much that is valuable and interesting is greatly to the credit of its authors; and its real importance as the most, indeed the only, elaborate discussion of that interesting seismic event which has thus far appeared, renders a careful examination of its methods and conclusions extremely desirable. I wish to remark upon a few points, concerning which I am compelled to dissent from the views expressed in the paper.

Great labor has evidently been expended on the construction of the isoseismal chart; and doubtless all has been done that can be, with the uncertain data available. Two serious but well-recognized difficulties are met with in attempting the construction of 'intensity' curves: one is the variability and inconsistency of the physical evidences of disturbance, and the other is the unreliability of human testimony as to its extent. In earthquakes of much violence a considerable area near the origin may present evidence which outlasts the disturbance itself, such as overthrown or damaged buildings, chimneys overturned, monuments displaced, etc., and which, therefore, may be studied at leisure. A little experience in the examination of this sort of evidence proves conclusively that a given result is an extremely complex function of a large number of 'independent variables,' most of which, unfortunately, are and must be unknown. It thus becomes difficult to determine the ratio between the varying magnitudes of any one of these variables, so largely is the visible result influenced by the others.

The available source of information consists generally of the effects of the disturbance upon structures of various kinds. Nothing can be more conflicting than the results of such observations, even on areas so small that it seems impossible to admit that differences in actual earth-movement have existed. Within a hundred feet of each other will be found buildings nearly destroyed, and buildings, apparently similar in construction, almost uninjured. Here a monument or shaft is overthrown; and there, a few feet away, another on a much less stable foundation is undisturbed. In a room in which heavy bookcases have been dashed upon the floor, and the furniture generally wrecked, delicate ornaments still rest upon the mantelpiece, and, without crack or scratch, seem to deny all possibility of violent motion. In short, one is forced to the conclusion that the character and amount of destruction caused by an earthquake depend largely on circumstances other than the motion of the earth-particle. An earthquake must be studied in the light of what it has failed to do, as well as of what it has done; and much consideration should be given to what might have happened but did not.

If such widely different effects can be produced by earth-movements which must be practically the same, it is clear that they cannot be very accurate measures of the intensity of seismic disturbances. In a general way, and if extended over an area which includes decided changes in the extent of surface destruction, such observations are extremely useful as indicating zones of unequal disturbance; and especially so, as, in the absence of instrumental records, they furnish about all the available facts.

The nature of the data furnished by the careful and conscientious survey of Mr. Sloan is not stated; but it is perfectly safe to say, that, whatever it may be, Messrs. Dutton and Hayden have made the most of it.

Without intending any special criticism upon the method of treatment adopted, I desire to call attention to the uncertainty, which seems to be great, in the construction of equal intensity-curves with any attempt at precision in form or position, when they are based upon observations of such physical disturbances as are referred to above.

If such records of the disturbance as are left by the earthquake itself are of doubtful and uncertain value, still more so must be the data resting entirely upon the testimony of observers of transient phenomena. It is by no means uncommon for two persons sitting in the same room, and disturbed by the same moderate earthquake, to differ decidedly in their estimate of its intensity.

In two differently constructed or differently situated buildings near to each other the difference is very great. Nor will it do to depend upon the disturbance of movable objects, such as swinging-lamps, etc. Very much depends upon the character of the movement, — as to whether the motion is principally horizontal or vertical, the period long or short, and the synchronism of that period with that of the moving object. Innumerable illustrations of this fact might be given. Disturbances of unusually large amplitude but long period are sometimes scarcely perceptible to the observer. Professor Milne recorded a disturbance in Tokyo on Nov. 23, 1884, of which he says, "Whilst standing up, it was with difficulty perceptible. In the same room, however, those who were seated felt it distinctly. It made a lamp six feet long swing through an arc about six inches."

In 1881 an earthquake occurred at Sapporo (Japan), concerning which the observer made this note: "Wire of hanging-lamp four feet long described an arc of twelve inches; not personally observed; was walking on the street, and nothing was noticed."

Besides the physical environment of the observer, his physiological and psychological peculiarities largely control his estimate of the extent of the disturbance.

In the collection of information by means of distributed circulars, it is impossible to avoid these difficulties, and to obtain any thing like a fair estimate of the character of the phenomenon, especially as most observers are inexperienced. A circular sent to a village is generally likely to find its way into the hands of the particular inhabitant who can give the most startling account of what he saw and felt, and who was naturally most thoroughly frightened.

It appears, therefore, that as far as the value of the collected data is concerned, the great area disturbed by this earthquake might be divided into three zones. The first is small, surrounding and including the epicentrum, and the visible evidences of the intensity of the shock were carefully studied by a sagacious observer within a few weeks of its occurrence. The second consists of the remainder of the area within two or three hundred miles of the epicentral tract, throughout which, though to a constantly diminishing extent, overthrown chimneys, displaced shafts, cracked walls, etc., remained as exponents of the character and magnitude of the disturbance. From this region, however, evidence

came through circular letters, newspaper reports, etc., with which untrained and not very trustworthy observers have much to do. The third zone consists of all that is left of the disturbed area: over it the effects were transient, and all evidence rests on human testimony, unsupported by that of material objects.

Thus it would seem, that, in the construction of the map, isoseismal lines would be drawn with three different degrees of confidence, and that they must be drawn more freely, and with less attention to detail, as they are farther removed from the epicentral tract. Local variations in intensity-estimates should have less weight, and the lines would approximate more nearly to smooth curves. On the map as drawn by Messrs. Dutton and Hayden, this order of things appears to be reversed: the smoothest, most regular curves are those immediately surrounding the epicentrum, while they become more irregular as the distance from that point increases.

In work of this kind, irregular and sinuous lines imply numerous and reliable observations, while those more regularly and uniformly curved will generally be drawn for areas over which observations are few, and not of sufficient weight to show more than the general trend of the line. For these reasons it appears to me that the map is faulty, in that too much weight has been given to individual observations at great distances from the epicentrum; that the sinuosities and irregularities in the lines, particularly those of the Mississippi valley, do not represent any thing real; that they should be smoothed out; and that it is doubtful if sufficient evidence exists for the construction of the two isolated areas, surrounded by closed curves, which appear in northern Illinois and in southern Indiana and Illinois. I venture to suggest, in regard to the latter 'area of silence,' that its existence may to some extent be due to the fact that information concerning that area was collected several months after the occurrence of the earthquake.

One of the most interesting features of this paper is the method employed in determining the depth of the seismic centre. Under certain restrictions, no criticism can be made upon the analysis of the problem; but in its practical application it is, in my judgment, open to serious objection. It is not easy to decide what is the best measure of the 'intensity' of an earthquake. A simple expression for it, and that accepted by Messrs. Dutton and Hayden, is 'the energy per unit area of wave-front.'

This definition once adopted, their analytical and graphic treatment of the problem is elegant and satisfactory; but in the application of the method to the Charleston earthquake, or to any other, it is important to ask whether any means exists for determining the 'intensity' as defined above. While it is true that the disappearance of the consonant a from the abscissa of the points of inflection renders it independent of the *absolute* intensity, it must not be forgotten that in determining *relative* intensities the thing to be kept in mind is 'the energy per unit area of wave-front.' As far as can be seen from the contents of the paper, the result depends on the unjustifiable assumption that *surface destruction* is proportional to this. It is a well-established fact that the destructive effects of a motion are not proportional to the energy involved, and in earthquakes many things combine to produce what is ordinarily called the 'intensity' of the shock, or, perhaps better, its 'destructiveness.' An exact expression for this is extremely desirable, and it seems to me that Professor Milne has approximated to it pretty closely in adopting, as he has, the 'maximum acceleration of the earth-particle in a horizontal plane.' There can be little doubt that horizontal movement is more effective in overturning and destroying buildings, chimneys, etc., than vertical; yet the fact, if it be a fact, finds no expression in the method of Messrs. Dutton and Hayden. Their formula and curve demand the maximum intensity at the epicentrum; and this is correct, according to their definition of intensity.

But does the greatest destruction take place at the epicentrum, or is it to be found in a zone whose radius depends on the depth of the seismic centre? I would not venture to place my own judgment, based upon a hasty examination along a single line, against that of an observer who has gone more leisurely over the field; but, as I can nowhere discover in the paper a distinct statement as to where the *most destructive effects* were observed, I may remark that it appeared to me that there was much less destruction in the neighborhood of the epicentrum, where the vertical component of the motion seemed to have predominated, than in and about the city of Charleston.

Of course, it is possible that from a study of the surface disturbance the relative amount of energy per unit area of wave-front at different points may have been worked out, and the point of inflection found from these results; but it would be an extremely complex problem, and, in addition to difficulties already suggested, it is complicated by the fact that the normal motion of the particle must be changed as the wave emerges from the earth: this, indeed, would stand in the way of getting just what is desired from perfect instrumental records, as, at best, they can only reveal surface movements.

I am unable to agree with the conclusion of Messrs. Dutton and Hayden expressed in the statement that the amplitude of vibration of the earth-particle was in some places not less than ten inches or a foot. So large an amplitude appears to me to be extremely improbable. It is only within a few years that any thing like accurate measures of amplitude have been made; and it is well known, that, wherever it has been measured, it has been found to be small.

In the 'general run' of Japanese earthquakes, the amplitude has been found to be not much greater than a millimetre, and often less. In a few cases it has been several millimetres; and I believe in one or two, which were nearly 'destructive,' and by which chimneys were overthrown and walls cracked, it has been as high as ten or twelve millimetres. It will be noticed, however, in examining these reports, that, in most of the cases in which large amplitudes are reported, the disturbances were of unusual length.

Although, in the construction of their numerous
Gray
'steady-point' seismographs, Messrs. Ewing (I want
Milne
to be careful not to put any one of these names first) have well-nigh revolutionized the science of seismology, I am inclined to the opinion that in a prolonged disturbance the 'steady point' is likely to be set in motion, and that a magnification of the amplitude may sometimes result. A very large

amplitude is to my mind incompatible with innumerable observations of *what did not happen* in Charleston. I admit the difficulty of the problem, but think it easier to account for large displacements by successive movements of small amplitudes.

I must also dissent from the opinion expressed as to the value of stopped clocks as a means of determining the time of the wave-transit. Is it not likely that most of the inconsistencies which appear on a comparison of such data arose out of the fact that many of the clocks were not correctly regulated to 75th meridian time, or that their errors were not known? The man whose clock or watch is 'just right' is met with at every turn, especially after an earthquake; but to most people this means that the error is not greater than a minute or two.

If all of the stopped clocks in the area disturbed had been in exact agreement before the shock, I do not think the errors would have been very great; except, perhaps, in the immediate vicinity of the source. The stopping of all clocks at any considerable distance probably occurred at the transit of the same great wave. Of course, a properly adjusted seismoscope with a clock attached is infinitely better, but I do not have great confidence in the 'observer with watch in hand.' Most intelligent observers in this country must be classed as inexperienced: the watch is not generally in his hand until after he is convinced that the something which has happened is an earthquake, and then it is very likely to have a large and unknown error. Should the disturbance be so considerable as to threaten to be destructive, the skill of the observer in 'measuring a part of the shock and estimating the beginning' is tolerably certain to be overshadowed by his disposition to seek a place of safety. The position and environment of the observer at the time of the occurrence will greatly influence the character of the phenomenon. As an illustration, I may compare my own observations with those of Professor Newcomb, when the Charleston earthquake was felt in Washington City.

I was seated in my library on the second floor of a three-story brick building, about four squares from the state, war, and navy building. As soon as the disturbance was felt, the time was noted. In a moment the motion became very strong. My small boy, who had been awakened out of a sound sleep, rushed into the room; and the family quickly decided to do what it had often done before under such circumstances, and found its way to the street. By the time this was accomplished all was quiet; and in two minutes from the beginning we were again seated in the same room, discussing the shock. In a few minutes, about five from the beginning, another shock occurred, much less violent than the first.

Professor Newcomb "observed a duration of perceptible tremors, with two maxima lasting about five and one-half minutes."

There is, of course, no doubt but what these tremors were felt, but it may be a question whether they were prolonged vibrations of the building in which Professor Newcomb was, or real earth-movements. I am pretty sensitive to earthquakes, and I can say with certainty that they were not felt by me or by my family.

Everybody, I am sure, will agree that it is highly important to establish a large number of observing-stations, equipped with the best instrumental appliances which can be obtained. Even so small a number as ten or twenty such stations, well distributed over the area disturbed by the Charleston earthquake, would have put us a long way in advance of our present knowledge of seismology. It is greatly to be hoped that the able and interesting discussion of the subject, which Messrs. Dutton and Hayden have evolved from the mass of observations which they have gathered with so much industry, will serve to direct the attention of intelligent people to the importance of such a system of observing-stations, and that in the near future the director of the geological survey will be enabled to establish it.

T. C. M.

Terre Haute, June 1.

Museums of ethnology and their classification.

The remarks of Dr. Boas and Professor Mason on the classification of ethnological material raise questions which must occur to every one who has before him unclassified material. As both views include a part of the truth, the decision on the course to be adopted must depend upon the amount of material to be handled, the space available for its exhibition, and the purpose most at heart in the organization of the museum considered as an agency for effecting a purpose.

The ideal way, if all circumstances were favorable, would be to have a double series,— one representing the culture of each people as an ethnic unit; and the other a comparative collection illustrating the relations to a common standard of the items making up each tribal aggregation. In ninety-nine museums out of a hundred, this would be impracticable, owing to the expense involved, the exhibition space required, and the difficulty of obtaining sufficient duplicate material for two series. The decision must therefore depend on the object to be attained. Is this to show the manner in which tools, weapons, dress, etc., have been elaborated, under the operation of the environment, by the human mind in varying stages of development, or is it rather to convey to the observer the resultant of all the forces acting in and on a comparable series of ethnic types or units, each complete in itself? In either case the object is a worthy one, and to be attained in its particular manner. Neither is likely to be completely attained under the existing conditions of museums in this or any other country; but, as attempted in different collections, we may regard them as complementing each other. In the one case, as very truly observed by Dr. Boas, we are helped to a knowledge of what problems exist; and it is no little matter to have a rational sailing-direction over a trackless ocean, though the accurate chart is still to be made. In the other, we have the equivalent of the monographic study of the specialist who surveys in detail, and for all time, a gulf or harbor forming a small part of the oceanic coast.

To conclude, for the people at large and the majority of those who profit by public museums, I believe the greatest amount of satisfaction and instruction is to be obtained rather from an ethnic arrangement than from the organic method; but this is merely an expression of my individual preference.

WM. H. DALL.

Washington, D.C., June 4.

Prof. Otis T. Mason's reply to my remarks on his views of the methods of ethnology is mainly a justi-

fication of his plan of arranging the collections of the national museum. As this plan is the outcome of his philosophical view of the problems of ethnology, we must scrutinize these in order to judge as to the merits of his system.

His principal object is the study of each and every invention among peoples of all races and countries. I am well aware that this idea was and is shared by many scientists; and at this very moment I read with interest Mantegazza's proposal of erecting a 'psychological museum,' i.e., a museum of ethnological objects arranged according to the ideas to which they belong. Professor Mason's rank among American ethnologists, however, and the weight he can give to his opinions by the arrangement of the large collections of the national museum according to his theories, induce me to criticise his views more particularly.

My view of the study of ethnology is this: the object of our science is to understand the phenomena called ethnological and anthropological, in the widest sense of those words,—in their historical development and geographical distribution, and in their physiological and psychological foundation. These two branches are opposed to each other in the same way as are biology and the so-called systematic 'organology,' or, as I have called it in another place (*Science*, ix. No. 210), when treating on the study of geography, 'physical science and cosmography;' the former trying to deduce laws from phenomena, the latter having for its aim a description and explanation of phenomena. I tried to show that both branches are of equal scientific value.

Let us inquire which method must be applied to carry on ethnological researches of either kind. Ethnological phenomena are the result of the physical and psychical character of men, and of its development under the influence of the surroundings: therefore two problems must be studied for attaining scientific results. The preliminary study is that of the surroundings: the final aim of the researches is the knowledge of the laws and history of the development of the physiological and psychological character of mankind. 'Surroundings' are the physical conditions of the country, and the sociological phenomena, i.e., the relation of man to man. Furthermore, the study of the present surroundings is insufficient: the history of the people, the influence of the regions through which it passed on its migrations, and the people with whom it came into contact, must be considered. All of these are phenomena which may directly be observed by a well-trained observer, or may be traced with greater or less accuracy by historical researches.

The second part of ethnological researches is far more difficult. The physical and psychical character of a people is in itself the result of the action of the surroundings, and of the way in which the present character was attained. Each stage in the development of a people leaves its stamp, which cannot be destroyed by future events. Thus it appears that the elements of the character of a people are extremely complex. There are two ways of treating this problem.

One of the remarkable features of such problems is the occurrence of similar inventions in regions widely apart, and without having a common origin. One method of studying them — and this is Professor Mason's method — is to compare the phenomena, and to draw conclusions by analogy. It is the deductive method. The other method is to study phenomena arising from a common psychical cause among all tribes and as influenced by their surroundings; i.e., by tracing the full history of the single phenomenon. This is the inductive method. For this method of study, the tribal arrangement of museum specimens is the only satisfatory one, as it represents the physical and ethnical surroundings.

I will explain these ideas by giving an example. It has frequently been proposed to establish a museum illustrating the adaptation of organisms to surroundings. The aim of this study is to find the physiological laws or the combination of causes which have the effect of causing these adaptations. The classification and arrangement must, of course, be made according to surroundings, in order to show their influence on different kinds of organisms.

An ethnological collection is analogous to this. The objects of study are researches on psychology. The method of researches is a study of the surroundings. The surroundings are physical and ethnical: therefore the arrangement must also be physical and ethnical, as this is the only way to show the single phenomenon in its peculiar character and surroundings.

It has been the tendency of science to confine the domain of deductive methods more and more, and not to be content with arguments from analogy, which are the foundation of most errors of the human mind, and to which may be traced the religious and other ideas of man in a primitive state of culture, and, to a certain degree, even in a state of advanced civilization. Science is constantly encroaching upon the domain of the argument from analogy, and demands inductive methods.

Nevertheless the psychological and scientific value of the argument from analogy cannot be overrated: it is the most effective method of finding problems. The active part it plays in the origin of philosophical systems and grand ideas which sometimes burst upon scientists is proof of this. But, as far as inductive methods can be applied,—and we believe that their domain will continue to increase,—induction must scrutinize the ideas found by deduction. Therefore I should call Professor Mason's system a suggestive one, but not fit for scientific researches, as it does not allow the application of the inductive method.

But even this acknowledgment must be limited. The technological idea, which Professor Mason has made the leading one in the arrangement of the collection of the national museum, is only one side, and a very limited one, of the wide field of ideas which must be leading in a 'psychological museum,' as Mantegazza calls it.

The rattle, for instance, is not merely the outcome of the idea of making noise, and of the technical methods applied to reach this end: it is, besides this, the outcome of religious conceptions, as any noise may be applied to invoke or drive away spirits; or it may be the outcome of the pleasure children have in noise of any kind; and its form may be characteristic of the art of the people. Thus the same implement belongs to very different departments of a psychological museum.

Furthermore, let us inquire what is the psychological principle upon which Mason's system is founded. The leading idea is technology. The foundation of technics is the faculty of acting suitably: consequently the purpose of the implement must be made the principle of division. For in-

stance, all kinds of cooking-pots and other arrangements for cooking would belong to one class. The mere fact that certain pots are made of clay would not justify the establishment of a pottery department. This quality of being made of clay is incidental, and does not agree with the psychological basis.

There is one point of view which justifies a classification according to inventions in a psychological museum. This is the extent to which each invention is used by a people: for instance, in what branches of life pottery is made use of, which may be limited in one tribe, very wide in another. But in this case the purpose of the object will not be the principle of division, but the principal invention applied in its manufacture; and thus the specimens would not be arranged according to Professor Mason's system, objects serving widely differing purposes belonging to one class. Therefore I cannot consider it justifiable to make technology, in the sense Professor Mason does, the basis of arranging ethnological collections.

One reason ought to make us very cautious in applying the argument from analogy in ethnology as well as in other sciences of similar character; biology, for instance. Former events, as I have already said, leave their stamp on the present character of a people. I consider it one of the greatest achievements of Darwinism to have brought to light this fact, and thus to have made a physical treatment of biology and psychology possible. The fact may be expressed by the words, "the physiological and psychological state of an organism at a certain moment is a function of its whole history;" that is, the character and future development of a biological or ethnological phenomenon is not expressed by its appearance, by the state in which it *is*, but by its whole history. Physicists will understand the important meaning of this fact. The outward appearance of two phenomena may be identical, yet their immanent qualities may be altogether different: therefore arguments from analogies of the outward appearance, such as shown in Professor Mason's collections, are deceptive. These remarks show how the same phenomena may originate from unlike causes, and that my opinion does not at all strive against the axiom, 'Like effects spring from like causes,' which belongs to that class of axioms which cannot be converted. Though like causes have like effects, like effects have not like causes.

From my statement it will be understood that I cannot content myself with Mr. Dall's remark, in the letter contained in to-day's issue, that both standpoints contain part of the truth. I have expressed in another place (*Verh. Ges. für Erdkunde*, Berlin, 1886, No. 7) my opinion on Dall's ethnological method, and emphasized, as I have here also, the necessity of studying each ethnological phenomenon individually.

In conclusion I have to add a few words on the practical side of the question upon which Professor Mason and Mr. Dall touch. In regard to this question, I concur with Mr. Dall, and believe that the public will be much more benefited by the tribal arrangement of ethnological collections.

I cannot agree with Professor Mason's proposal of arranging the cases like a checker-board. In ethnology all is individuality. We should be compelled to leave long rows of cases empty, as certain phenomena occur but in very few tribes. It would be almost impossible to show in this way all important ethnological phenomena, the historical development of tribes, the influence of neighbors and surroundings, etc. It is my opinion that the main object of ethnological collections should be the dissemination of the fact that civilization is not something absolute, but that it is relative, and that our ideas and conceptions are true only so far as our civilization goes. I believe that this object can be accomplished only by the tribal arrangement of collections. The second object, which is subordinate to the other, is to show how far each and every civilization is the outcome of its geographical and historical surroundings. Here the line of tribal arrangement may sometimes be broken, in order to show an historical series of specimens; but I consider this latter point of view subordinate to the former, and should choose to arrange collections of duplicates for illustrating those ideas, as it were, as an explanation of the facts contained in the tribal series. Of course, it is generally impossible to do this, on account of the lack of specimens, or, more frequently, on account of the lack of our knowledge; but it is my ideal of an ethnological museum. I wish to state here again that I am not at all opposed to Mantegazza's psychological museum, which will be very suggestive and important for the development of science, but I consider the ethnological museum indispensable for controlling the ideas suggested by the analogies shown in the psychological collection, and as the only means of showing the state of culture of man.

DR. FRANZ BOAS.

Correlation of the geological structure of the maritime province of Canada with that of western Europe.

I take the liberty to send a corrected abstract of a paper read by me before the Royal society of Canada, and which may perhaps be of interest to some of your readers: —

As early as 1855, in the first edition of 'Acadian geology,' the author had indicated the close resemblance in structure and mineral productions of Nova Scotia and New Brunswick with the British Islands, and in subsequent editions of the same work further illustrations were given of this fact. Recent researches by Bailey, Matthew, Fletcher, Ells, and others, had still more distinctively indicated this resemblance, as well as the distinctness of the maritime geology from that of the great interior plateau of Canada and the United States. In short, as argued by the author in his recent address before the British association, the geology of the Atlantic margins of America and Europe is substantially the same, and distinct from that found west of the Appalachians in America and in central and eastern Europe. In this fact has originated much of the difficulty experienced in correlating the geological formations of eastern Canada with those of Ontario, of New York and Ohio, as well as similar difficulties in Europe which have led to much controversy and difference of classification and nomenclature. One object of the present communication was to show that the system of classification of paleozoic sediments employed for the interior plateau of the American continent requires very important modifications when applied to the Atlantic coast, and that neglect of this has led to serious misconceptions.

The rugged islands of Laurentian and Huronian rocks correspond in both regions, and show an identity of succession in deposits as well as a synchronism of the great folds and lateral pressures which have disturbed these old formations on both sides of the Atlantic. The Cambrian sediments and fossils as originally described by Hartt, and more recently and in so great detail by Matthew, are in close correspondence with those of Wales, and not identical with those of internal America. The recent paper of Lapworth on the graptolites affords evidence of the same kind, and shows that these were Atlantic animals in their time. It also throws much additional light on the Quebec group of Logan, considered as an Atlantic marginal formation, representing a great lapse of time in the Cambrian and Ordovician periods. The author had long ago shown that the Siluro-Cambrian or Ordovician of Nova Scotia conformed more nearly to that of Cumberland and Wales than to the great limestone formations of Quebec, Ontario, and New York. The upper Silurian also is of the type of that of England and Wales, — a fact very marked in its fossil remains as well as in its sediments.

The parallelism in the Erian or Devonian in both countries is most marked, both in rocks and fossils; and, while this is apparent in the fishes as worked up by Mr. Whiteaves, it is no less manifest in the fossil plants as described by the author.

The carboniferous, in its limited troughs, the character of its beds, and its fossil animals and plants, also points to a closer relationship in that period between the two shores of the Atlantic than between the Atlantic coast and the inland area. This was evidenced by comparative lists of species.

The trias of Nova Scotia and of Prince Edward Island, as the author had shown in 1868 (*Journ. geol. soc. Lond.*), resembles that of England very closely in its aqueous deposits and in its associated trappean rocks.

Beyond this, the geology of the maritime provinces presents no materials for comparison till we arrive at the bowlder drift and other pleistocene deposits. In regard to these, without entering into disputed questions any further than to say that the observations of the author, as well as those more recently made by Mr. Chalmers, conclusively proved that submergence and local ice-drift were dominant as causes of distribution of bowlders and other material, there was evidence of great similarity. The marine beds described by Mr. Matthew at St. John were precise equivalents of the Clyde beds of Scotland, as were the upper shell-bearing beds of Prince Edward Island and Bay de Chaleur of those in Aberdeenshire and other parts of Scotland, and the Uddevalla beds of Sweden. The bowlders drifted from Labrador to Nova Scotia were the representatives of those in Europe scattered southward from Scandinavia, and the local drift in various directions from the hills was the counterpart of that observed in Great Britain. The survival of Mastodon giganteus in Cape Breton, to the close of the pleistocene, is a decided American feature, and so is the absence of any evidence of pleistocene man.

The conclusion of the author was, that, in so far as paleontology and the subdivisions of systems of formations are concerned, the geology of the maritime provinces is European, or perhaps more properly Atlantic, rather than American, and is to be correlated rather with the British Islands and Scandinavia than with interior Canada and the United States. The latter country, even on its eastern coast, possesses a much less perfect representation of these Atlantic deposits than that in the maritime provinces and Newfoundland; though the recent studies of Crosby, Dale, and others are developing new points of this kind in the geology of New England, and Hitchcock and others have shown that the New Brunswick geology extends into Maine.

The paper further discussed the bearing of these facts on the successive stages of the physical geography of eastern America in the Cambrian, Silurian, Erian, carboniferous, and triassic records.

J. WM. DAWSON.

Montreal, May 30.

Sea-sickness.

In *Science* for June 3, I find a very interesting review of the medical literature of this subject. It is but natural that means, both prophylactic and curative, should be sought for the benefit of those who find a sea-voyage one of torment rather than pleasure; and the writer has frequently thought that some suggestions derived from the otologist's experience might not be without interest in this connection. Thus, in a considerable experience among persons suffering from aural disease, it has been found that vertiginous symptoms are of frequent occurrence; that the phenomena, in fact, which constitute what is known as 'sea-sickness,' are by no means exclusively experienced by the comparatively few who submit to being tossed about at sea. Indeed, as every one familiar with the subject very well knows, most of the symptoms going to make up this malady are found, in some form or other, to render the lives of a great many persons living upon *terra firma* most miserable. A great many of these individuals experience almost daily, frequently much oftener, sea-sickness without ever going on board ship. The sufferings of these seem to be owing to a faulty condition of the transmitting mechanism of the ear, — defects in respect to which it may be said, that, when normal tension of this portion of the hearing-organ is thus wanting, nearly all the symptoms of sea-sickness may take place from slight though altogether unavoidable, constantly occurring causes. Persons thus affected cannot rise up suddenly from a recumbent position, or otherwise change the pose of the head, without feeling dizzy or staggering when attempting locomotion. Sometimes they experience nausea, and feel faint and otherwise miserable. Or the mere acts of swallowing, yawning, or hiccoughing, whereby intra-tympanal aeration is suddenly altered, may be followed by distressing and sometimes alarming symptoms. The experience of vertiginous phenomena in some form or other, closely simulating what is known as 'sea-sickness,' likewise occurs to the aurally defective in consequence of cerebral concussion caused by impacts of the stapes upon the fluid in the labyrinth, and arising from oscillatory movements of the drum-head when its functions are no longer under the dominance of normal tension. The erratic drum-head, flapping in response to sudden movements of the head, acts of swallowing, etc., would seem to force the stapes into and out of the oval window to an extent far exceeding its physiological limits; and, thus jostled about, the stapes, with each excursion of the drum-head, imparts a shock to the labyrinthine fluid. I am aware that it has long been held by physiologists

that the disturbances of equilibrium which I have above attributed to concussion, are due to some specific functional disturbance in the semicircular canals; but observations drawn from a study of a large number having anomalies of the drum of the ear, lead me to exclude that theory. It is true of the aurally vertiginous just described, that they represent chronic forms of ear-disease, and are usually neuropathic subjects beyond middle life. But similar cerebral disturbances are not unusual at any age in acute inflammation of the middle ear. Other things being equal, elderly persons are less obnoxious to sea-sickness than the young, since the latter are much more susceptible to impressions upon the nervous system. A friend of the writer who has made many ocean-voyages was always a great sufferer in this regard in early life, but in after years experienced but little inconvenience in the roughest weather. On one occasion, however, a berth was assigned him in the after part of a vessel, when, after experiencing for a short time the discomforting concussions arising from the motions of the screw, he became dreadfully sea-sick while lying in bed. The distress becoming unbearable, he was removed to a berth amidships, when recovery was almost immediate. It is well known that persons at the beginning of a voyage may become quite sea-sick, and yet entirely recover before landing, — an experience probably due to the bracing effect of sea-air. It will be seen, that, regarded from the point of view afforded by an aurist's clinical experience, nearly all of the phenomena of sea-sickness may be said to occur on shore, in consequence of cerebral (labyrinthine) concussion, especially during a state of nervous exhaustion. Sea-sickness would seem to be brought about in most instances, irrespective of aural defects, from the agitation of the cerebro-spinal fluid caused by the motions of a vessel at sea, as has already been described by other writers. Of course, the concussive impact from tossing upon the waves is usually very gentle, but its long continuance finally overcomes the resisting power of the subject. The effect may be to make one tired or sleepy only, but too often nausea and dreadful depression are experienced. As in auditory concussion, such symptoms as a sense of constriction or of pain and great tension in the head characterize the more severe cases arising at sea. It is seldom that the landsman experiences the uninterrupted jarring of the brain which must be endured at sea; but the writer has seen many cases where the despondency from the concussion of sound even, as well as the other causes before mentioned, was almost as great as could be endured.

Where so many conditions favor the occurrence of sea-sickness, it is scarcely to be hoped that any specific cure will ever be found. In the writer's own experience, the nitrite of amyl, properly employed, has often been found to relieve some of the more disagreeable symptoms, through its influence on the vaso-motor system. SAMUEL SEXTON.

New York, June 8.

Two balloon-voyages.

The two hundred and fiftieth anniversary of the founding of the city of Providence, R.I., afforded an opportunity for making meteorological observations in the free air. Mr. Hazen of the signal office, Washington, D.C., volunteered his services, and was accepted. On June 24, with a light east-north-east breeze and a gentle rain, the balloon City of Boston left Providence at 5.35 in the afternoon. There were four persons on board, which made it a little crowded; but by leaning out of the basket it was found possible to make the observations, which consisted chiefly in readings of an aneroid barometer, a sling psychrometer, and a watch. The balloon passed over Fishville, Hope, Coventry Centre, and West Greenwich, R.I., and landed in the tree-tops of Voluntown, Conn., at a little after 7.30. The temperature, on leaving the earth, was 60°.2, and at no part of the voyage did it reach a point below 56°.7. The highest point reached was 850 feet, at 6.43. An interesting observation on this voyage was the continual rising and falling of the balloon without the expenditure of ballast. This was partly due to the following: 1. A momentum acquired by the balloon was checked when the drag-rope (about seven hundred feet long) left the earth. Then the balloon began to descend till sufficient weight of the rope on the ground gave it enough buoyancy to rise: this, in turn, was counterbalanced as before. 2. A rise in the balloon was accompanied by a slight fall in temperature: this affected the gas, and gave it less buoyancy. On the other hand, a fall brought the balloon into warmer air, which had a tendency to reverse the former effect.

On June 25 the veteran aeronaut, James Allen of Providence, R.I., and Mr. Hazen, made a voyage, starting from the landing-place of the night previous. The air was perfectly still, and while there was no rain falling, yet the appearance of a heavy fog or mist hung rather low on the hillsides. It was impossible to discern any motion in this mist or in clouds above it.

The start was made at 7.44 in the morning, the air temperature being 61°.3. It was decided to make as high an ascent as possible. The earth was lost sight of at about 1,160 feet. The lowest temperature in the cloud was 58°.3, at 1,670 feet; and from this point it rose rapidly to 65°.6, at 2,450 feet. The highest point reached was 9,780 feet, at 9.18, with a temperature of 48°. Having been out of sight of land more than an hour, and the proximity to sea being rather close, it was deemed prudent at this point to make a descent, which was done with great rapidity; the basket striking the earth with some force, having fallen the 9,700 feet in thirteen minutes, or at the rate of twelve feet per second. The balloon landed within about two and one-half miles of the point from which the ascent of the previous day was made. The temperature at landing was 64°.2, with a gentle north-east wind. At a height of about 8,400 feet the shadow of the balloon was seen upon the clouds, with two rainbow-colored rings about it. Besides the interesting observations of temperature, indicating a rise of over eight degrees in an ascent of eight hundred feet, and showing that just at the top of the cloud the temperature was abnormally high, there were also observations on the direction of the balloon above the clouds. It has been usually considered that above the clouds it is impossible to tell any directions. It was found, on throwing over dried leaves, that they took a definite direction as shown by the compass, and afterward it was found that the balloon was moving in the direction which was indicated by the observer, or slower than the leaves. At the time this observation was made, the balloon was slowly rising, and it

would seem always possible to ascertain the direction under these circumstances. The clouds presented a magnificent spectacle, and seemed like gigantic billows upon a boundless ocean. The sun was very hot indeed, and every effort was made to observe a rising motion in the cloud, but entirely without success. Observations of humidity were made with a sling wet bulb, and the air temperature by a thermometer with a bulb about two millimetres in diameter. All the experiences indicated, that, with modern appliances of drag-rope and anchor, ballooning is entirely safe, and is especially adapted for researches in the free air, which are so very important at the present stage of the science of meteorology. H. A. HAZEN.

Washington, Aug. 13, 1886.

The freezing-point of sea-water.

I submit the following as the result of several very careful determinations of the freezing-point of sea-water, made at North Bluff, Hudson Strait (latitude 62° 33′ 45″ north, longitude 70° 41′ 15″ west).

The situation of the place of observation was within an inlet, at about a mile from its mouth, into which the waters of the strait had unlimited access. A stream twenty feet wide discharged into the inlet at its head, two miles away.

The determinations were made on March 4, 1885, when the temperature of the air was —12°.6 F., in the following manner: —

A hole about four feet square having been cut through the ice (2.85 feet thick), the water within it was thoroughly agitated by stirring from below, and during the actual operation slightly agitated. The thermometer was held nearly horizontally, the bulb slightly lower than the rest of the instrument, just below the surface of the water. When the ice film began to form at the surface of the water, the corrected reading of the thermometer (Negretti and Zambra, No. C. 3456) was 26°.7 F., at which point it remained stationary; so that, under the conditions I have mentioned, the freezing-point of sea-water is 26°.7 F., — a point very much lower than that usually accepted as its freezing-point, and differing from it in a direction contrary to what we should have expected from the generally accepted assumption that northern waters are of less specific gravity than more equatorially situated waters.

It would have added to the value of the result had I obtained the specific gravity of the water at the time. Later, when I thought to have done so, unavoidable circumstances prevented my doing so.

I might add that a similar determination was made on the opposite shore of the strait with a very closely agreeing result. W. A. ASHE.

The Quebec observatory, June 7.

The scientific swindler again.

The following from one of the local papers here will show that the peculiar person who has repeatedly been shown up in *Science* is still at large and at work: at least, I presume he is the same person, since it is unlikely that there is more than one such perverse genius abroad. This time he turns up as a deaf-mute, attached to the Smithsonian, and named 'R. M. Vasile.'

"The Syracuse (N. Y.) *Herald* says, 'A highly educated man, who appeared to be deaf and dumb, and who represented himself to be an attaché of the Smithsonian institution at Washington, came here eight or ten days ago, and succeeded in ingratiating himself into the confidence of Prof. W. A. Brownell of the high school, and of other scientific gentlemen. He gave his name as R. M. Vasile. It took him but a short time to prove himself a master of geology, mineralogy, and chemistry, and his proficiency in those sciences lent color to his representation that he had come here to investigate the rocks and minerals of Onondaga county, and also to get together material for a report on its fishes. Professor Brownell obtained from him for a mere trifle a rare and valuable scientific work, and for one dollar and twenty-five cents got a promise from him, that, upon his return to Washington, he would send on a set of trilobites. Having thus won the confidence of the professor, he began to talk of exchanging specimens with his new-made friend; but his offers excited suspicion, and an inquiry sent by telegraph to Washington brought back the information that Vasile was not in the government's employ. Soon afterward the man disappeared, and he has not been heard from since. He left a board-bill at the Kingsley House, and the impression prevails there that he only pretended to be deaf and dumb. His scheme is apparently to borrow books and scientific specimens in one town, and dispose of them in another.'"

ELLIOTT COUES.

Smithsonian inst., June 8.

SCIENCE.—SUPPLEMENT.

FRIDAY, JUNE 17, 1887.

ORIGIN OF PUEBLO ARCHITECTURE.

NEARLY twenty years ago, Lewis H. Morgan called attention to the false views of aboriginal American civilization then current. His remarks were intended to apply particularly to the higher cultures of Mexico and Central America, which had always been interpreted through the medium of the glowing accounts of the Spanish conquerors, who saw in every institution some parallel to their own customs. The resulting exaggerated views of Indian culture have thrown a reflected light upon the architectural remains of the south-west. The deserted pueblos scattered over a large portion of New Mexico and Arizona, and extending far into Utah and Colorado, have been linked with the name of Montezuma and the Aztecs by the early pioneers; and the fact that our first knowledge of these remains reached us through such sources doubtless had much influence in fixing erroneous ideas of the ancient builders. These deserted groups of carefully built stone houses, occurring in the midst of desert solitudes, appealed strongly to the imaginations of the early explorers, and stimulated their fancy to reconstruct an elaborate civilization, and to connect the remains, on such slender basis, with their vague notions of the 'Aztecs' and other mysterious peoples. This early implanted bias has caused the invention of many ingenious theories concerning the origin and disappearance of the builders of the ancient pueblos. They have been regarded as a remarkably advanced people, who were swept from the face of the earth by some mighty catastrophe. Their 'buildings' have been said to 'equal any in the United States, if we except the Capitol;' and many more equally absurd extravagances have been uttered in connection with the ruined houses of the ancestors of the present Pueblo Indians.

The work of the bureau of ethnology in our south-western territories has included an examination of a great many of these ruins, and a comparison with the existing pueblos. In connection with the latter portion of the work, many traditions bearing on the occupancy of the ruins by their ancestors have been secured from the present Pueblo tribes, connecting them clearly both with many of the old village ruins and with the cliff-dwellings. A number of these ruins are the remains of villages that have actually been occupied within the historic period. Both the architectural and traditional evidence are wholly in accord in establishing a continuity of descent from the ancient Pueblos to the present time, many of the present tribes being made up of the more or less scattered but inter-related descendants of clans who in former times occupied the villages whose remains are looked upon to-day as the homes of 'Aztec colonies,' etc.

The complete adaptation to the peculiar environment displayed by this system of architecture would indicate that it had long been practised under the same conditions that now prevail in this region, and which still affect the building-methods of the modern Pueblo Indians. A vast number of these pueblos have been constructed of the tabular sandstone found in natural quarries at the bases of hundreds of cliffs throughout these tablelands. This stone naturally breaks into small pieces of regular form, suitable for use in the simple masonry of the pueblos without any previous artificial treatment. The walls themselves give an exaggerated idea of the regularity of the component stones, owing to the care and neatness with which these are placed. The photographs taken in connection with the bureau's work among the ruins show clearly that the material of the walls was not nearly so regular as the appearance of the finished masonry would suggest, but that this finish depended on the careful selection and arrangement of the fragments, with the best face of each stone placed outwards. In the case of some of the best-finished masonry, the photographs indicate that the *core* of the wall has been laid up with the larger and more irregular stones, and the surface afterwards brought to a finish by carefully filling in and chinking the joints with smaller stones and fragments, sometimes not more than a quarter of an inch thick; the whole surface finally being reduced to a uniform face by rubbing the wall with a slab of sandstone.

Although many details, both of construction and arrangement, display a remarkable adaptation to the physical character of the country, yet the influence of physical environment alone would not suffice to produce the architectural type under consideration. Another element is necessary to give point and direction to such influence, in order to develop the results we find. This element was the *necessity* for *defence*. There are many evidences that the Pueblo population of these south-western tablelands have been subjected to the

more or less continuous operation of this *defensive motive* throughout the period of their occupation of this territory. A strong and independent race of people, who had no invasions of stronger foes to fear, would have been necessarily influenced by the environment to the extent of using the exceptional materials offered, and would have progressed in perfecting their lodges; but the motive for building clusters of rectangular cells — the initial point of departure in the development of the pueblo system — would not have been encountered. The crowding of many habitations within the narrow limits of a small cliff-ledge or other restricted site, bringing about the rectangular room-cluster, would most likely have been due to the imperative conditions imposed by this necessity for defence. The character of many sites occupied is not such as would be selected voluntarily by a people in a low grade of culture, and the choice of such places as homes must have been largely compulsory.

The general outlines of the development of this system, wherein the ancient builders were stimulated to the best use of the exceptional materials about them both by the difficult conditions of their semi-desert environment and by the necessity for constant watchfulness and protection against their neighbors, can be traced in its various stages of growth from the primitive conical lodge, and culminating in the large communal village of a single many-storied building, such as we find on the Chaco and also in the homes of some of the present Pueblo tribes. Yet the various steps have followed from a very simple and direct use of such material as was immediately at hand, with gradually improving methods of employing the same, as the experience derived from frequent experiments in building taught them to more fully utilize local facilities, the builders doing the best they could with the materials at hand. In all cases such material was derived from the nearest available source; and the occasional variations in the quality of the finished work were usually due to variations in the quality of the stone near by, or other local features.

The results accomplished attest the patient and persistent industry of the ancient builders, but the work does not display any evidence of great skill in construction or in the preparation of the material.

The same semi-desert environment that furnished such an abundance of material for the ancient builders, also, from its difficult and inhospitable character and the constant variations in the water-supply, furnished the conditions for compelling the *frequent use* of this material; and this was a most important factor in bringing about the degree of advancement in the building art that was attained. At the present day, constant *local* changes occur in the water sources of these arid tablelands, while the general character of the climate remains unchanged.

The pueblo system of construction, then, may be regarded as the product of the defensive motive, operating through an environment that furnished at the same time both an abundance of suitable building-material and the climatic conditions that compelled its very frequent employment.

The comparative abeyance, within the past few years, of the defensive motive, which has been such an important element in the evolution of this building system, has left its impress on the more recent architecture. Even after the long practice of the system has rendered it somewhat fixed, comparative security from attack by their neighbors has caused many of the Pueblo Indians to recognize the inconvenience of a system of dwellings in such large clusters, and on sites difficult of access, while the sources of their subsistence are necessarily sparsely scattered over large areas. This is noticeable in the construction of single houses of small size at quite a distance from the main villages, the motive of greater convenience to crops, flocks, water, etc., being allowed to outweigh the defensive motive.

The greater security of the Pueblos as the country comes more fully into the hands of Americans, has resulted also in the much more careless methods of construction, as well as of arrangement, that characterize the modern examples as compared with the ancient.

It seems altogether likely, that, as time goes on, the system of building a great number of rectangular rooms in many-storied clusters will be gradually abandoned by these people, in the absence of the defensive motive that bound them together and was the compulsory cause of such construction; and a more convenient system of scattered small houses, located near springs and fields, will take its place, thus again returning to a plan of living that must have prevailed at one period in the past evolution of the pueblo, prior to the clustering of a great many rooms into one large defensive village.

The apparently distinct line of separation between the Pueblo Indians and the neighboring tribes gradually becomes less clearly defined as further investigation makes both sides better known and reveals many connecting links. Mr. Cushing's exhaustive study of Pueblo social, political, and religious systems has clearly established their essential identity with those of other tribes. In the sphere of the arts, where the wid-

est discrepancies apparently occur, it is found, that, by tracing the development of each branch of Pueblo art by means of its own internal evidence of the successive periods of growth through which it has passed, we establish its continuous evolution from the simplest beginnings. Mr. W. H. Holmes has clearly shown how the ceramic art of these peoples has naturally developed from the simplest sources, and such as were more or less common to most of the American aborigines in a comparatively low stage of culture. In the case of their architecture, a similar derivation from very primitive forms can be traced. The builders gradually learned to utilize their environment, and perfect the system, until it culminated in the many-storied fortress-pueblo of a single building (such as the ruined pueblos of the Chaco); yet these highest achievements of their art in building contain within themselves a record that these people at one time dwelt in simple circular lodges, such as were common to many American tribes at the period of their discovery.

VICTOR MINDELEFF.

GEOLOGY OF NEW JERSEY.

UNDER the wise and efficient management of Professor Cook, the very modest annual appropriation of the geological survey of New Jersey is made to yield, year by year, substantial contributions to the geology of the state. The report for 1886 shows that the admirable topographic survey of New Jersey, carried on by the state in co-operation with the U. S. geological and coast and geodetic surveys, is approaching completion. It is being published on a scale of one mile to the inch; and the sheets for the northern part of the state, which were issued some time ago, have been generally accepted as the finest piece of cartographic work, for so large an area, that has been done in this country. They are in constant demand for all the uses requiring an accurate horizontal and vertical delineation of the surface of the country, from laying out water-works and railroads to arranging bicycle tours.

In view of the substantial benefits already accruing from this map before its completion, the wisdom and practical importance of such work cannot be questioned; and it is to be hoped that other states will hasten to profit by New Jersey's enlightened example.

The results of this topographic survey are to be used, on a reduced scale, as the basis of a new geological map of the state.

In the purely geological part of this volume, Dr. Britton's chapter on the crystalline or primitive rocks of New Jersey occupies a prominent place. Three conformable groups are recognized: 1. Massive group, composed chiefly of indistinctly bedded syenitic and granitic or gneissic rocks, and probably equivalent to the Ottawa gneiss or lower Laurentian of Canada; 2. Iron (magnetite) bearing group, embracing a great variety of gneissic and schistose strata poor in white mica, sparry limestone and dolomite, with graphite and serpentine, and bedded deposits of magnetite, franklinite, and other ores (this group agrees well with the Grenville series or upper Laurentian of Canada); 3. Gneissic and schistose group, including biotite and garnetiferous gneisses, mica, hornblende, talc, tremolite, cyanite, chlorite, and other schists; vein granite, bedded diorite, and impure limestone and serpentine. This group resembles Dr. Hunt's Montalban system; and, since it is conformable with the iron-bearing group, the view is advanced that the Montalban may be simply an upper division of the Laurentian. It is interesting to note here that other students of the great Appalachian belt of crystalline strata have been led to propose more or less similar re-arrangements of the crystalline terranes, all of which goes to show the extremely unsettled state of eozoic geology. Dr. Britton introduces a series of sections to show that the same conformable sequence of his three groups obtains in all parts of the highland district; but in view of the massive character of the first group, and the general paucity of outcrops at critical points, this view can scarcely be regarded as definitely established.

It has long been known that the rocks of the highlands, like those of the Appalachian belt generally, are involved in a series of closely appressed folds the axial planes of which are usually inclined at a high angle to the south-east. This report, however, brings out more clearly than ever before, another important feature of these folds; viz., that their axes are not horizontal, but are inclined at an average angle of thirty degrees to the north-east. Since the pitch of the folds is always in the same direction, this involves a series of transverse faults with the uplift on the north-east; and more or less important examples of such faults have already been observed, especially in the iron-mines.

Among the paleozoic strata of this region, none are more interesting, or have proved more puzzling to geologists, than the red conglomerate and associated limestone and slate composing the Green Pond Mountain Range. In the earlier reports of the survey these were referred to the Potsdam, Trenton, and Hudson River groups. The later investigations, however, have resulted in the accumulation of proof, both stratigraphical and paleontological, that these rocks belong much higher in the scale; the red conglomerate being the equiva-

lent of the Oneida, the horizon to which Mather referred it forty years ago, the limestone being clearly of lower Helderberg age, while the slates are shown to belong to the Hamilton group. The Medina, Oriskany, and corniferous groups are also recognized here, and the entire thickness of this great outlier is estimated at 2,750 feet.

Perhaps no formation in this country, equally simple in origin and structure, has provoked so much discussion as the triassic of the Atlantic seaboard. The principal problems which it presents, it is well known, are the monoclinal dips of the strata, and their exact relations to the associated masses of trap. As regards the first, geologists are now pretty generally satisfied that the uniform inclination of the beds is not due to their original deposition on a sloping surface, but to faulting or some similar subsequent disturbance. But, while the studies of Prof. W. M. Davis on the triassic of the Connecticut valley have greatly strengthened the view that the trap sheets of that region are mainly contemporaneous lava-flows, regularly interstratified with the sandstones, Professor Cook is unable to accept this explanation for the trap ranges of New Jersey, holding that they are mainly intrusive and subsequent to both the deposition and disturbance of the sandstone. It is satisfactory, however, to observe that both observers are obliged to qualify the expressions of their views by using the word 'mainly,' which really makes the difference one of degree only; and it may very well be that the trap is more generally intrusive in the one field than in the other, or the exposures of the trap may be more favorable for showing its intrusive aspect in New Jersey and its contemporaneous aspect in New England.

The surface geology is described under the heads of 'glacial drift' and 'yellow gravel.' The former characterizes the surface of the northern quarter of the state, and the latter of the southern three-quarters. The problems of the age and origin of the yellow gravel are discussed at some length, but not satisfactorily solved.

The concluding chapters on economic geology treat of the iron and zinc mines, the cretaceous and tertiary marl-beds, water-supply, and drainage.

CHALLENGER REPORT.

Three enormous volumes, aggregating over eighteen hundred pages and one hundred and forty plates, represent the contribution of the Challenger expedition to the scientific knowledge of this attractive group. The reporter, Prof. E. Haeckel of Jena, has devoted some ten years to the study of the collection, and his work forms the largest single report of the whole series.

Report of the scientific results of the exploring voyage of the Challenger. Vol. xviii.: Radiolaria. London, Government. 4°.

The Challenger expedition found Radiolaria universally distributed throughout the ocean, and their skeletons nearly equally wide-spread over its bottom; their relative abundance and species differing in different localities, and these differences being correlated with some of the most interesting and intricate problems of general oceanography. It was fortunate, as observed by Dr. Murray, that so distinguished a naturalist should have been willing to undertake a task so laborious and lengthy as the examination of the thousands of minute forms obtained by the Challenger. Professor Haeckel, as will be seen by the most cursory examination of the plates, was extremely fortunate in having the co-operation of Mr. Adolf Giltsch, who made all the drawings of the sixteen hundred new 'species' figured for the report.

The Radiolaria are marine rhizopods, whose unicellular body always consists of two parts, — an outer calymma, which has no nucleus and from which the pseudopodia extend; and, separated from this by a membrane, an inner capsule with one or more nuclei, serving as the special organ of reproduction and general organic centre. Digestion and relations with the outer world in general are attended to by the calymma, and the distinguishing feature of the class is furnished by the constant capsule-membrane separating the two layers. The radiolarians are usually furnished with a skeleton which presents the greatest beauty and utmost variety of form, and is generally composed of silica, or, in certain cases (Acantharia), of an organic substance called 'acanthin.' The individuals are usually single: in only a small minority are the unicellular organisms united in colonies or caenobia.

A systematic catalogue, which forms the termination of the work, and includes all the Radiolaria known up to 1884, contains twenty 'orders,' eighty-five 'families,' seven hundred and thirty-nine 'genera,' and four thousand three hundred and eighteen 'species.' It is hardly necessary to say that these groups have no such value in terms of organization as those in common use by systematists for higher groups of animals. Professor Haeckel's attitude toward systematic biology is analogous to that of an anarchist toward the civil law, and, like that, if adopted by all naturalists, would be likely to result in an indefinite number of individual despotisms. The multiplication of names and groups, apart from their value in relation to other organisms, is pretty well justified by the enormous number of differentiable forms described. It is more than probable, also, in the absence of discriminative natural selection operat-

ing among these multitudinous lowly organisms, that what is recognized among higher animals as specific differentiation, cannot exist, any more than among the foraminifera. So, for the purpose of marshalling, in some sort of order, the chaos of individuals, perhaps nothing better could have been chosen than the arrangement adopted.

The richest source of the material described is the radiolarian ooze of the Pacific Ocean, the remarkable deep-sea mud consisting chiefly of the skeletons of these animals. The tow-net also yielded rich treasures. Professor Haeckel has also included the fruit of his own numerous journeys to the Mediterranean and the eastern Atlantic as well as to the Indian Ocean. Capt. Heinrich Rabbe of Bremen also contributed most important material from the Indian seas; and the collections of Murray and others on various expeditions, such as the Knight-errant and Triton voyages, added to the total. The alimentary canal of various pelagic organisms and even Jurassic coprolites have been laid under contribution. Dr. R. Teuscher of Jena has co-operated with the author in his work: among other things he undertook the tedious micrometric measurements, some eight thousand in number, by which the constancy of the so-called specific forms was endeavored to be tested. The result showed their inconstancy, as might be expected. The conclusion of Professor Haeckel that all other organisms exhibit a similar inconstancy, is, we believe, not in accordance with the general experience of naturalists.

No description can do justice to the wonderful variety and beauty of these minute creatures, and for fuller realization the reader must turn to the plates of what we may properly call this stupendous undertaking.

FOURTH ANNUAL REPORT OF THE BUREAU OF ETHNOLOGY.

THE present volume, which has just been issued, contains the report of the director for 1882–83, and some papers of eminent value. The latter must be reviewed separately, and we shall confine ourselves to some remarks on Major Powell's report. The broad basis on which the researches of the bureau are carried on is due to him, and ethnologists must be thankful for his encouragement of special lines of study — for instance, Mallery's researches on sign-language and pictography — and of special researches on certain groups of tribes, which cannot be made without the assistance and support of a powerful institution. In this respect the work of the bureau is of the greatest value, as it puts an end to the dilettanteism which formerly obtained in American ethnology. Major Powell's attempts to gain the co-operation of scientists not officially connected with the bureau cannot but exert a wholesome and encouraging influence on American ethnology. Numerous valuable researches which are included in the reports of the bureau and in the contributions to North American ethnology are proof of this.

Another important feature of the work of the bureau is the broad and systematic plan by which Major Powell carries on the researches of the bureau. He keeps three publications particularly in view. His remarks on this subject are of great interest. He contemplates the publication of, "1°, a series of charts showing the habitat of all tribes when first met by Europeans, and at subsequent eras; 2°, a dictionary of tribal synonymy, which should refer the multiplied and confusing titles, as given in literature and in varying usage, to a correct and systematic standard of nomenclature; 3°, a classification, on a linguistic basis, of all the known Indians of North America, remaining and extinct, into families or stocks.

"The order of possible preparation of these publications is the reverse of the above. The charts cannot be drawn until the tribes, as villages, confederacies, and leagues, shall have been resolved from multiplicity and confusion into identification and simplicity. The linguistic classification precedes the whole of the work, and the difficulties attending it have at times suspended its satisfactory progress until expeditions of research had been sent forth to clear up the obstacles of uncertainty and ignorance. Numerous publications of ethnologic charts of partial synonymes and of tentative classifications have appeared from various sources, but all have been imperfect and more or less erroneous. The personal attention of the director and of all the officers and employees of the bureau has been steadily directed, in addition to the several branches of work from time to time undertaken, to presenting them in a proper form. The labor and study required have been beyond expression, but may be partially indicated by the fact that, apart from the linguistic and sociologic problems involved, the mere mechanical compilation has produced over twenty thousand cards of synonymy. The present condition of this interconnected work is encouraging." The publication of this material will be the first sound basis of continued researches on American ethnology. We do not enter into the details of the field-work done by the bureau, as during the subsequent years much additional work has been done, and has become known in its outlines. In this respect it must particularly be regretted that these reports, like most other government publications, are not sooner issued.

We heartily concur with Major Powell, in his remarks on the undesirability of amateur collectors and travellers. Unfortunately, many explorers are so little conversant with the elements of ethnology, and so little able to consider natives from any other point of view than that of our own civilization, or to enter into their methods of thinking, that they do more harm than good. Any one who has studied ethnological literature knows how true this is. It is an underestimation of private work, however, when Powell says, "Experience has shown that individual travellers, unguided and without common system, have failed to obtain the best results in examining members of native tribes both as individuals and as aggregations." This affirmation is opposed to the encouragement of private researches, which Powell has so successfully made the policy of the bureau. We do not doubt that scientists who are supported by the moral influence and the means of the bureau have better chances of success than those who travel without such support; but, as the bureau of ethnology is not able to carry out all the field-work that is necessary and desirable, researches of scientists undertaken outside of the systematical plan of the bureau ought to be welcome.

We consider the plan by which the researches of the bureau are carried on a very successful one. The principal idea is that the phenomena of ethnology and archeology must be studied from a common point of view, and that a knowledge of the former is indispensable for understanding the latter, and that the supposition of sudden cataclysms, instead of that of a continuous development, is only justified where clear evidence of the occurrence of such phenomena can be shown. The work of the bureau is of great importance not only for science, but also for a successful method of making the Indian a useful member of the state and of human society. We cannot press upon him our civilization. A thorough knowledge of the Indian character is necessary to reach satisfactory results in this line. Both scientists and philanthropists must wish that the work of the bureau be carried on as vigorously as possible, and that its operations ought not to be hampered by lack of means for extensive field-work and publications.

DR. FRANZ BOAS.

THE ROTIFERA.

IN our previous notice of this work (vol. vii. p. 402) we based the favorable judgment, which we then expressed, upon the first two parts. We have now before us the completed work, the examination of which strengthens our previous favorable opinion. The authors are not of those whose studies are prompted by an insatiable eagerness for knowledge, but rather, it appears to us, are they lovers of Nature, who seek the closest intimacy with her to gratify their affections. They are pleased to quote upon the reverse of their titlepage Shelley's lines: —

> "Those viewless beings,
> Whose mansion is the smallest particle
> Of the impassive atmosphere,
> Enjoy and live like man."

We do not mean that the characterization of the species is vague and dreamlike. It would be difficult for a biologist to determine the systematic position of Shelley's 'viewless beings' from the poet's description; but Mr. Hudson's are scientifically exact, although they are rendered interesting by the addition of something of the literary flavor that alone is present in Shelley's beautiful inexactitude. It is this combination of qualities which imparts a double merit to Hudson and Gosse's monograph, and renders it acceptable and welcome alike to the professional and to the amateur naturalist.

The work is a valuable contribution to science, as every conscientious monograph must be; for it is indispensable to progress that we should have from time to time, in regard to a given subject, a comprehensive presentation of the accumulated knowledge. A monograph of the Rotifera was very much needed, for it is twenty-five years since the revision by Dr. Arlidge. To execute the task worthily, it was necessary that the many, by no means always rare, species which had remained undescribed should be properly investigated, so as to be included in the monograph. This laborious undertaking the authors have accomplished. Their work contains more than one hundred and twenty species which were unrecognized when Dr. Arlidge wrote: nearly all of these have been added to science by the authors themselves, some eighty of them by Mr. Gosse.

When Mr. Hudson passes beyond his *rôle* of observation and description, and occupies himself with problems of morphology and of the affinities of the Rotifera, he is less fortunate than we could wish. Thus, he says in his preface that his discovery of the remarkable Pedalion mirum "has put beyond question the fact that the Rotifera, in one point at least, are closely linked to the Arthropoda." Now, Pedalion is a true rotifer, which has six limb-like appendages, two of which are on the median line (one being dorsal, the other ventral), and four of which are lateral. The limbs have terminal bristles. These appendages impart, in

The Rotifera; or, Wheel-animalcules. By C. T. HUDSON, assisted by T. H. GOSSE, F.R.S. Parts iii.–vi. London, Longmans. 8°.

fact, something of a Nauplius-like appearance to the animal; and, inasmuch as the Nauplius is the larval stage of certain Crustacea, Pedalion may be said to offer some resemblance to an arthropod. It must be remembered that arthropod limbs are always symmetrically disposed, and never occupy a position in the median line, except as a secondary modification resulting from the fusion of two originally distinct limbs into one median structure; as occurs, for example, in the Labium. Moreover, arthropod limbs are the appendages of segments, and are arranged in serial order lengthwise of the body and by segments. In the Rotifera, on the contrary, there is and can be no such arrangement, because there are no segments. In fact, we must interpret the similarity — which, after all, is imperfect — of the limbs of Pedalion to those of the Nauplius as an analogy, and not as an homology.

So, much may be said to indicate the limit beyond which the special merits of the work do not extend; but within those limits we find a great deal of the best excellence, which abundantly justifies our congratulating the authors upon the completion of their capital and thorough treatise.

LETTERS TO THE EDITOR.

[Continued from p. 592.]

The cause of consumption.

THIS subject is of such great importance not only in the prevention but also in the treatment of the disease, that I feel sure you will permit me to reply to the important objection raised by 'Medicus' to my theory of consumption. In science we proceed from the known to the unknown. Now, we know that the constant inhalation of small particles produces consumption, and that they evidently reduce the breathing capacity; and we have produced experimentally the disease in animals by simple confinement, which also reduces that capacity. Further, I have produced consumption by reducing the breathing surface of the lungs below a certain point, and I have searched the records in vain to find a case of consumption in which such conditions were not present. The tribes that are absolutely free from this disease are known to live under conditions that tend to develop the lungs; and we see the introduction of civilization amongst them — that is, of conditions that tend to reduce the breathing surface — is followed by the introduction of that disease. But, says 'Medicus,' — and I have had the same objection here, — that is because the bacillus has been introduced. I reply, apply the same process of examination to the bacillian theory, and it fails at the very beginning. Koch's important experiments — they mark an epoch in the knowlege of life — resulted in an apparent affirmative and an absolute negative. In some animals he induced consumption, in others he did not. What is the difference between the two classes of animals? The former evidently had been, and were, subjected to conditions that tend to reduce the breathing capacity; while the latter had not been, and were not, subjected to such conditions to the same extent. What followed the stoppage of the ventilating shafts of several wards at Brompton, an outbreak of consumption? No. Erysipelas. In civilization we do not know where the bacillus, so called, tuberculosis is not, and I am curious to see who will prove their absence amongst the tribes that are yet free from consumption. And while the germicide treatment of the disease has admittedly failed, that based upon this theory has, both in the experiments and in the four cases to which it has been applied, proved completely successful.

G. W. HAMBLETON.

London, May 25.

Scandinavian studies in the United States.

The readers of *Science* had their attention directed to this subject in a recent article written by Daniel Kilham Dodge; but the writer of that article, unwittingly I suppose, does injustice to the Scandinavians in this country as well as to the work that is so nobly being carried on by them. He also omits a prominent university in the north-west which is trying to do what he thinks ought to be done by many American colleges. As to the success of such efforts, his historical account has important lessons.

He states that there is "a population of 107,768 Scandinavians in Minnesota, and there is not a college in which the parent tongues of this great mass of people can be studied."

This might convey a wrong impression about the Scandinavians, if the readers of *Science* were not informed that during the year 1886 between seven and eight hundred students attended the Scandinavian institutions of Minnesota. True, these institutions are not as yet complete colleges in the American sense of the term, but the day is not far distant when some will be an equivalent. Their object is not degrees, but qualifications. These people have been nurtured by European universitiy principles, and with university men in their midst: they are not slow in fathoming the shallowness of a great deal of the American college-training.

Gustavus Adolphus college, situated at St. Peter, Minn., is a flourishing institution with two hundred students, that is lacking only one year of having a four-years' collegiate course. One-half of the professorships are held by men who are not Scandinavians, but Americans educated in eastern American colleges. Latin, English, German, mathematics, and natural sciences are taught by these professors. Augustana college, Rock Island, Ill., is another and older institution, supported by the Swedes, which has been graduating class after class for a period of ten years. Persons holding a diploma from this latter institution are admitted into the University of Upsala without examination. A goodly number of the professors are also American college-bred men. Within recent years a most promising educational work was begun by the Swedes at Lindsborg, Kan. During the past year, over three hundred students attended the different departments of Bethany college and Normal institute, and at the coming commencement they will dedicate an elegant and large college-building.

The Swedes and the Norwegians are alive on educational matters, and their influence is and will continue to be felt in this country. They are Swedes

and Norwegians, and no one can blame them if they desire their children to be educated in a way that they can appreciate it; and, if the Americans can not and will not do it, they will and must do it. As a rule, they are not opposed but glad to have their youth learn English; but they also wish them to know something more, especially the language, literature, and history of the fatherland. The complaint made against them often comes from denominational headquarters, because they cannot proselyte them fast enough. The Scandinavians are Lutherans, and they will resist any and every attempt that is made to rob them of the faith for which Gustavus Adolphus fought and died. Allow them the religious liberty of which we boast as Americans, and they will be Americans too.

J. P. Uhler.

St. Peter, Minn., June 2.

Thomson's electrostatic voltmeter.

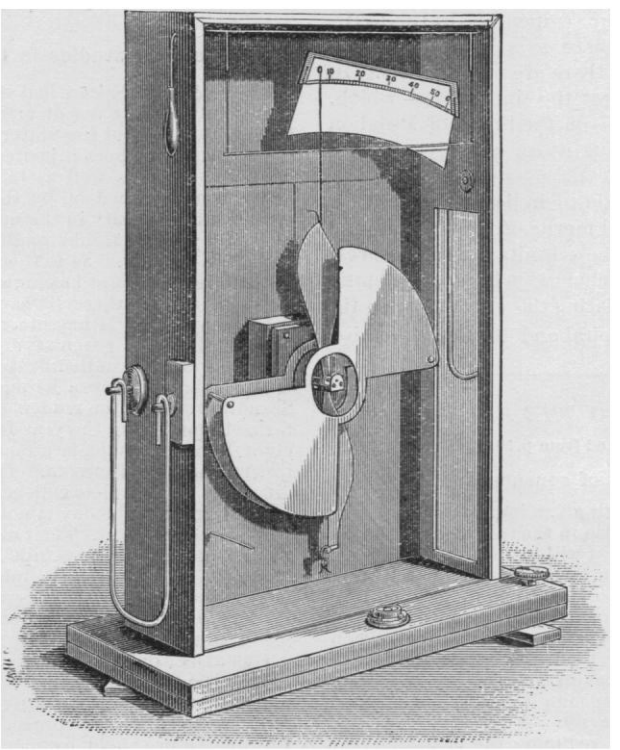
Respecting your inquiry as to the merits of the Thomson electrostatic voltmeter, I must say that I have made great use of it during the last year, and am very much pleased with its performance. It has the disadvantage of not being very portable, and I fear that the wood of which the enclosing box is formed will go the way of all European woods in our climate. I begin to see evidence of warping now, which will make it necessary to re-examine the scale of the instrument.

The instrument will, however, hold its charge remarkably well, evidence of which I have recently had, in which I have used a large battery of Leyden jars as a source of electricity.

The instrument measures between four hundred and ten thousand volts, and is exceedingly useful in connection with the Holtz machine and other high-tension sources.

F. E. Nipher.

St. Louis, June 3.



THOMSON'S ELECTROSTATIC VOLTMETER.
(Reproduced by permission of James W. Queen & Co.)

The report recently issued by the geological survey of Kentucky, on the geology of Elliott county, discusses the coal-measures of that region, and especially the massive conglomerate, which, along certain uplifts, has been deeply trenched by the streams, the vertical walls of the narrow and exceedingly picturesque gorges ranging from 75 to 175 feet in height. We also find here full accounts by Messrs. Crandall and Diller of the trap dike of Elliott county, which is noteworthy as being the only mass of eruptive rock yet discovered in Kentucky, and of the 'interesting possibilities' in the way of diamonds suggested by Professor Lewis. But, although this peridotite is similar to that so closely associated with the diamonds in South Africa, Mr. Diller finds no facts which would warrant a persistent search for the gems in Kentucky.